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Tackling BEPS in the Global South: Evidence from Peru's tax reform

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ABSTRACT

This study assesses the effect of a transfer pricing reform on tax payments at the firm level. Given the critical role that the consulting firms play in tax avoidance schemes, we include the effect of expenditure on tax advisory. Exploiting the reform's particular features, we use a regression discontinuity design to estimate the causal effect of the tax reform. We find that firms affected by the intervention on average paid more taxes in 2017. Although we do not find effects for 2018, we find no conclusive evidence that spending on tax advisory drove such an effect.

1. Introduction

Raising tax revenues is essential to finance development in Peru. In Latin America and the Caribbean (LAC), 15.6% of this income comes from multinational companies (OECD, 2020). However, corporate tax abuse practices such as base erosion and profit shifting threaten the collection of those revenues. The loss of income in developing countries due to these practices is estimated at USD 200 billion annually (Cobham and Janskỳ, 2018; Crivelli et al., 2015). To address this problem, countries introduce transfer pricing rules in their legislation to generate transparency for intra-group transactions and those carried out from, to, or through tax havens by multinational companies.

This study analyzes the effect of the transfer pricing tax reform implemented in Peru in 2017 on firm's tax payments. In Peru, transfer pricing regulation has been in force since 2001, and in 2017, a tax reform was implemented based on the OECD/G20 Base Erosion and Profit Shifting Project (from here on, the BEPS Actions Plan). This reform strongly increased the reporting requirements on intra-group and tax haven transactions, introduced new deductibility criteria for expenses arising exclusively from intra-group services, and strengthened the monitoring of international transactions by increasing the number of specialized tax auditors devoted to these tasks.

This study also evaluates the effect on tax advisory spending, which is a channel firms use to reduce their tax payments. In fact, consulting firms play a prominent role in tax avoidance schemes (Chyz et al., 2021; Jones et al., 2018; Donohoe and Knechel, 2014; Dhaliwal et al., 2013; McGuire et al., 2012). Companies could invest in sophisticated tax planning schemes to avoid paying taxes. For this reason, even in the presence of fiscal transparency instruments, we analyze the effect of the tax reform on tax advisory spending.

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We use a regression discontinuity design (RDD) to exploit the rule given by the tax reform in which firms receive an intervention according to their level of revenues and transactions with related firms. We provide several robustness checks to validate our identification strategy. After conducting manipulation and placebo tests, we find that the policy increased tax payments for firms near the cutoff point and that this effect was significant in the year in which the reform came into force, 2017. However, the impact is not significant in a later year. Furthermore, we find little evidence to support the idea that firms affected by the policy invested more in tax advisory to avoid paying taxes.

The contribution of this study is the following. First, there is scarce literature on the effect of transfer pricing rules in developing countries (Kumar et al., 2021), even though these countries are affected most in terms of income loss through BEPS practices (Johannesen et al., 2020; Cobham and Janskỳ, 2018). Second, previous empirical studies (Rathke et al., 2020; Yoo, 2020; Beer and Loeprick, 2015; Marques and Pinho, 2016) focused on the effect of the intervention and did not explain the drivers that could help firms continue to avoid paying taxes. Therefore, we incorporate spending on tax advisory into the analysis. Firms could use advisory services to purchase more sophisticated tax planning services to help avoid paying taxes to companies. Finally, the study also seeks to contribute to the growing debate on international taxation (Zucman, 2019; Coppola et al., 2021) as an effective way to increase tax revenues and tax progressivity and, ultimately, reduce inequality (Piketty and Cantante, 2018). This paper is closest to Bustos et al. (2022), who also studied the effect of a price transfer reform that significantly increased documentation requirements on tax revenue. In addition, while they conducted a qualitative study on the role of tax advisory services, we employ quantitative analysis to study the effect on tax revenue.

The paper is structured as follows. The next section briefly outlines the context of base erosion and profit shifting in developing countries. The third section explains the tax reform that Peru implemented. The fourth and fifth sections present the empirical strategy and the data used, respectively. The subsequent section discusses our results, and the final section concludes with a discussion of questions for further research.

2. Context

Developing countries face a dilemma when dealing with multinational companies. On the one hand, multinational companies are essential to increase innovation, investment and productivity in these countries. Hence, governments should minimize the tax burden on these companies in order to encourage foreign investment (Spencer, 2008; Harrison and Rodríguez-Clare, 2010; Kose et al., 2010; Alfaro and Chen, 2018). On the other hand, multinationals tend to have more possibilities and resources to avoid taxes compared to national companies that cannot access international networks. Previous research (Huizinga and Laeven, 2008; Beuselinck et al., 2015; Johannesen et al., 2020) has shown that subsidiaries are controlled by their parent companies to carry out transactions with other subsidiaries to minimize the payment of taxes at the economic group level. Multinational companies carry out certain transactions that shift their earnings from subsidiaries in high-tax countries to low-tax countries. These are the so-called BEPS practices.

As economies grow, the number of multinational companies operating in their territories tends to increase, and the question of how to monitor base erosion and profit shifting gains importance (Johannesen et al., 2020). The literature shows that these practices are essential for developing economies. Cobham and Janskỳ (2018) indicate that the highest losses due to BEPS practices occur in low and low-middle-income countries in sub-Saharan Africa, Latin America and the Caribbean, and South Asia. They estimate that these losses represent 2% of total tax collection in OECD countries and between 6% and 13% in developing countries. Moreover, Crivelli et al. (2015) estimate that the loss of global tax revenue is around USD 650 billion annually, of which around a third corresponds to developing countries. Regarding Peru, Garcia-Bernardo and Janskỳ (2021) show the loss of tax revenue due to BEPS to be estimated at 2.92% of total tax revenues.

The OECD designed a package of reforms of the international tax system to overcome this issue, the BEPS action plan, which set 15 specific actions to ensure international tax rules fit an increasingly globalized and digitized business world. The actions also aim at preventing multinational companies from paying little or no tax (OECD, 2015). From this package, only actions 8, 9, 10 and 13 are focused on transfer pricing and aim to increase the transparency of intra-group transactions to tackle profit shifting and base erosion.

The literature analyzing the effect of transfer pricing regulation on tax payments is incipient in developing countries. Wier (2020) investigated the impact of an OECD-recommended transfer pricing reform implemented in South Africa. In 2012, policymakers introduced several measures to limit transfer mispricing through increased documentation requirements and audit discretion. The author found that this reform did seem to limit transfer mispricing in 2012–2014, but it returned to its original level in 2015.

The literature on the effects of transfer pricing regulation in developed countries is profuse and varied. Bustos et al. (2022) analyzed a transfer pricing reform based on OECD recommendations implemented in Chile. The reform consisted of more reporting requirements, accountability and resources invested in enforcement. The authors found no effects on tax payments and no reduction in the propensity to shift benefits to low-tax countries. Rathke et al. (2020) found that this regulation inhibits profit-shifting practices, that some transfer pricing rules can be more effective than others, and that companies can manipulate their transfer prices even in the presence of this regulation. Beer and Loeprick (2015) found that the estimated profit shifting between subsidiaries of multinational companies is reduced by 52% two years after introducing the obligation to present transfer pricing documentation. However, they did not find a significant effect in affiliates with high intangible endowments. Marques and Pinho (2016) showed that stricter transfer pricing regulation is associated with a lower tax-gap sensitivity of reported earnings. Finally, Beuselinck et al. (2015) analyzed the collective impact of transfer pricing rules with other laws on profit shifting. Their results indicated that firms shift profits from high-tax to low-tax countries when applying the weak local regulatory framework. As can be seen, the literature on transfer pricing rules in developing countries is scarce and indicates that the effects of this regulation are not long-term, while the effects in developed countries have been widely studied, with diverse results. In that regard, the first research question that this study seeks to answer concerns the effect of the transfer pricing tax reform that was implemented in Peru in 2017 on tax payments at the firm level.

In spite of global tax reforms, multinationals seem to continue to pay low taxes. Given this, the literature has studied the role that tax consulting and audit firms play in tax avoidance schemes in order to investigate the channels that could be used by multinational companies to reduce their tax payments. Jones et al. (2018) studied the impact of the services of Big 4 firms on the activity of multinational companies in tax havens. They found that there is a strong correlation and causal link between the size of a multinational company's tax haven network and its use of audit services provided by Big 4 firms. Furthermore, McGuire et al. (2012) investigated whether a firm's experience in auditing and tax matters influences the level of tax avoidance of its clients. Their results suggest that clients who hire tax consulting and financial auditing services from the same firm incur greater tax avoidance, suggesting that firms with experts in both subjects can combine their tax and auditing experience to develop tax strategies that benefit its clients both from a tax and a financial statement perspective. Chyz et al. (2021) found that companies that hire their auditors for tax compliance services and tax planning incur greater tax avoidance and less exposure to risk compared to companies that do not hire their auditors for tax compliance work. Likewise, tax avoidance results are more pronounced for companies whose auditors have more tax experience and seniority in the position, as well as for companies with greater fiscal and operational complexity. Finally, Bustos et al. (2022) analyzed the effect of the transfer pricing reform in Chile on the tax payments of firms. While they found that after the reform there was an increase in the demand for tax advisory services in transfer pricing matters, they found no effects on the payment of taxes. They conducted in-depth interviews with tax consultants and found that consulting firms sold tax planning services to their clients beyond transfer pricing tax compliance work. As can be seen, there is evidence of strategies and tactics used by tax consulting firms to sell schemes that enable their clients to avoid paying taxes. In that regard, the second research question that this study seeks to answer concerns the role tax consulting firms played in implementing the transfer pricing tax reform that was carried out in Peru.

Peru offers an interesting institutional framework to analyze the effect of a tax reform on increasing tax revenue collection. First, Peru is a developing country that recently initiated a tax reform to face base erosion and profit shifting through transfer pricing legislation to join the OECD. There is no empirical evidence on the effectiveness of this tax reform. Second, Peru has one of the lowest tax-to-GDP ratios in the Latin American and Caribbean (LAC) region. The country registers a tax-to-GDP ratio of 16.6%, well below the OECD average of 33.8%, and even below average for Latin America and the Caribbean (22.9%), being one of the most lagging countries in the region (OECD et al., (2021)).

3. Peruvian tax reform

Before the tax reform, in 2016, the regulation regarding transfer prices forced firms with controlled transactions (CT)[1] to provide a partial Local Report. The partial Local Report provides basic information about the related firms, the economic transactions between them, and the methodology for the chosen prices.

The Peruvian tax reform on transfer pricing came into force on the 1st of January 2017 and included actions 10 and 13 of the BEPS Actions Plan. The reform strongly increased the reporting requirements on intra-group transactions, introduced new deductibility criteria for expenses arising exclusively from intra-group services, and boosted the monitoring of international transactions by increasing the number of specialized tax auditors devoted to these tasks. The following paragraphs explain how actions 10 and 13 were implemented in the Peruvian tax law.

Action 10. The tax reform introduced the following obligations for all firms that carry out CT regardless of the amount and level of income:

- Benefit test. Firms must comply with the benefit test and provide specific documentation requested by the tax authority as necessary conditions for the deduction of expenses regarding intra-group services. The documentation provided must show the effective provision and nature of the service and the real need for it, as well as the costs and expenses incurred by the service provider and reasonable criteria for allocating these.
- Approach for low value-adding intra-group services. Under Peruvian law, these services (i) are supportive in nature, (ii) are not part of the core business of the multinational group or company, (iii) do not require the use of unique and valuable intangibles or lead to the creation of unique intangibles and valuable intangibles, and (iv) do not involve the assumption or control of substantial or significant risk by the service provider or result in the creation of significant risk for the service provider. In addition, Peruvian law establishes that intra-group service providers will apply a profit margin of up to 5% on the total costs and operating expenses incurred in the provision of their services.

Action 13. The tax reform introduced the following obligations for firms that meet certain thresholds for income and CT.

- Local File: Firms with annual revenues over 2300 UIT[2] (US$ 2.86 million in 2017) and positive CT. Firms with less than 400 UIT of CT must present a partial Local Report. This requirement was already in place in 2016. Firms with more than 400 UIT must present a complete Local Report, which involves providing information about the structure of the organization, board of directors, shareholding, the nature of the business, main clients and providers, business restructuring, and more details of the CT such as the specific context of this operation.

[1] Controlled transactions (CT) are operations performed by the taxpayer with related parties or from, to or through countries or territories with low or zero taxation. Controlled transactions include revenues and expenses.

[2] UIT is the acronym for *Unidades Impositiva Tributarias*, the Peruvian term for tax units. UIT are different each year: In 2016, 1 UIT was S/ 3950 or $ 1170; in 2017, it was S/ 4050 or $ 1242; and in 2018, it was S/ 4150 or $ 1263. "S/" refers to the currency sign for Peruvian Soles.

Table 1
Application of the tax reform according to the income threshold and controlled transactions.

	CT = 0	0 < CT < 400 UIT	400 UIT < CT
Revenues > 2,300UIT	No applicable action	Action 10	Action 10 Action 13
Revenues < 2,300UIT	No applicable action	Action 10	Action 10

Source: Gobierno del Peru (2016).

- Master File and Country by Country Report: Firms with CT over 400 UIT must present these reports that contain basic information about the MNE with which the company is transacting and information regarding the global allocation of income and taxes of the MNE in each country where it is carrying out transactions.

Table 1 summarizes the applicability of actions 10 and 13 between firms' revenues and CT.

4. Empirical strategy

We use a sharp regression discontinuity design (RDD) to disentangle the causal effect of tax reform on firms' tax payments. In the following, we explain why this quasi-experimental method is valid for our purposes and develop the empirical model.

In our setting, an RDD is justified for the following reasons (Imbens and Lemieux, 2008; Lee and Card, 2008). First, the tax reform has an assignment rule that is clear and followed with high fidelity. The assignment rule of this policy is that firms with an annual revenue above a threshold of 2300 UIT are affected by the reform because the cost of undertaking BEPS practices is significantly higher.[3] The assignment or running variable is the total annual firm revenue.

Second, the running variable is an ordinal measure with sufficient density on either side of the cutoff point. Near the cutoff, there are 1712 firms, with 1185 receiving the treatment and 527 not receiving the treatment.

Third, firms should not anticipate the policy or manipulate the value of the running variable. If firms near the cutoff reduce their total annual revenue in order not to be subject to tax reform, the RDD is invalid. However, it is unlikely that firms could have anticipated the law enacted in January 2017 and the exact information they had to report in mid-2018. Moreover, if firms manipulated their running variable, there should be a heap just below the cutoff in the histogram. According to Fig. 1, there is no clear bunching in firms near the cutoff (red line). There are fewer firms with a revenue below S/ 5 million of the cutoff. Nonetheless, this did not seem to be part of the systematic behaviour of firms wanting to avoid the tax reform, otherwise firms would heap below the cutoff and not above, like the figure shows.

We conduct manipulation testing procedures using the local polynomial density estimators proposed by Calonico et al. (2018) in the spirit of McCrary (2008) to test whether there is a significant difference between the density estimation and the confidence interval just below and above the cutoff. If the density estimation is higher or lower than the cutoff, it would suggest that firms manipulate their running variable to avoid the tax reform. Fig. 2 shows that the local polynomial density estimate and confidence interval are reasonably continuous near the cutoff. When performing the statistical manipulation test, we do not reject the null hypothesis of no manipulation as the p-value is 0.6632. Therefore, we do not find evidence of manipulation in the running variable.

Finally, in Peru there was no other government measure tailored to firms whose annual revenue was at least the considered threshold from 2016 to 2018. We develop balance tests and other falsification tests in the Appendix.

We now develop the empirical specification formally. Let Y_i be the outcome of firm i. Let $Y_i(1)$ be the firms' outcome when they satisfy the eligibility of the policy and $Y_i(0)$ be the outcome when they do not satisfy the requirement. All eligible firms received the treatment. D is the treatment variable that takes a value of 1 if the firm received the treatment and 0 otherwise. Moreover, let X_{it} be the annual revenue of the firm i, and c be the cutoff point. We assume that for $D = \{1,0\}$, then $E[Y_i(D)|\, X_i = x]$ if a function that is continuous at $x = c$ (continuity condition). The sharp RDD estimator is given by:

$$\beta_{RD} = \lim_{\epsilon \to 0} E(Y_i(1)|\, X_i = c + \epsilon) - \lim_{\epsilon \to 0} E(Y_i(0)|\, X_i = c - \epsilon)$$

$$\beta_{RD} = E(Y_i(1) - Y_i(0)|\, X_i = c) \tag{1}$$

β_{RD} is the local average treatment effect as the running variable approaches the cutoff in the limit. Therefore, our study only has internal validity, as the results are valid for firms near the cutoff range. Provided that firms just below and above the cutoff are similar and that the assignment variable is continuous, we estimate the expected value above and below the threshold using kernel-weighted local polynomial regressions (Hahn et al., 2001). This method is more robust than standard non-parametric regression because it reduces the bias in boundary-point estimation. Moreover, kernel-weighted local polynomial regressions are superior to parametric higher order polynomial estimation because they reduce the bias introduced by overfitting (Gelman and Imbens, 2019). Further, we include covariates because they can increase the efficiency of the estimator (Calonico et al., 2019). In particular, we estimate the following econometric specifications,

[3] We used the yearly updated UIT in the analysis for different years. For example, we consider the UIT established in 2018 for the effects studied in 2018.

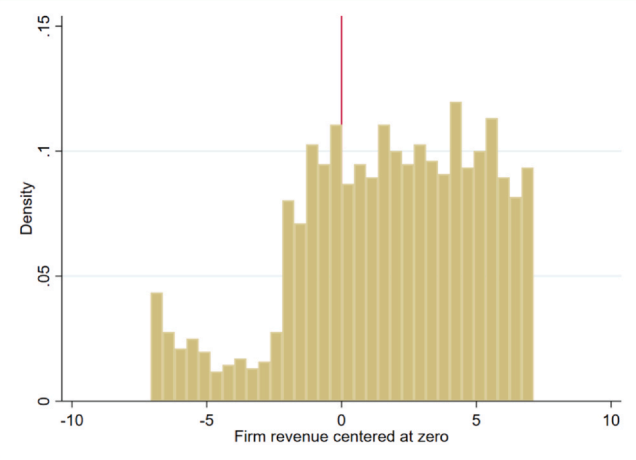

Fig. 1. Histogram on firm's revenue around the cutoff, 2017. *Notes:* Firm's revenue is measured in millions of Peruvian Soles. It was subtracted from the value of the cutoff point centered at zero by which firms are subject to the tax reform. The cutoff is S/ 9.315 millions (around US$ 2.86 millions) in 2017. Only firms within the optimal bandwidth and with positive controlled transaction operations are shown.
Source: INEI (2021a).

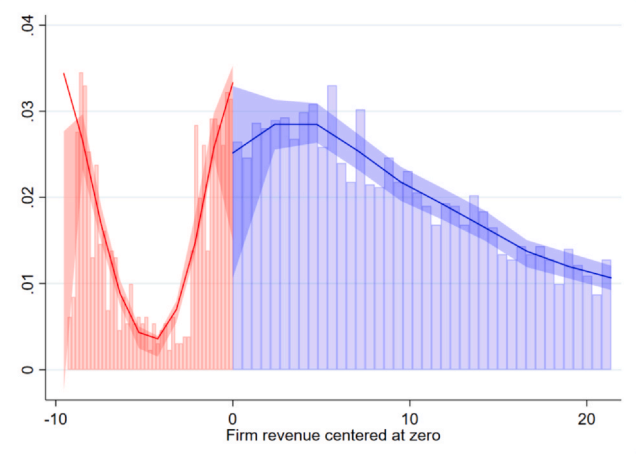

Fig. 2. Manipulation testing plot. *Notes:* Firm's revenue is measured in millions of Peruvian Soles. It was subtracted from the value of the cutoff point centered at zero by which firms are subject to the tax reform. The cutoff is S/ 9.315 millions (around US$ 2.86 millions). Only firms with positive controlled transaction operations are shown. Local polynomial density estimate and robust bias corrected confidence intervals are computed following Cattaneo et al., (2018) procedure.

$$(\hat{\alpha}^+, \hat{\beta}^+, \hat{\gamma})^+ = \underset{\alpha,\beta,\gamma}{\operatorname{argmin}} \sum_{i=1}^{n} 1(X_i \geq c)(Y_i - \alpha - \beta(X_i - c) - \gamma Z)^2 K(\tfrac{X_i - c}{h})$$
$$(\hat{\alpha}^-, \hat{\beta}^-, \hat{\gamma}^-) = \underset{\alpha,\beta,\gamma}{\operatorname{argmin}} \sum_{i=1}^{n} 1(X_i < c)(Y_i - \alpha - \beta(X_i - c) - \gamma Z)^2 K(\frac{X_i - c}{h}) \quad (2)$$

Hence, the empirical RD estimator $\hat{\beta}_{RD} = \hat{\alpha}^+ - \hat{\alpha}^-$. $1(X_i \geq c)$ is an indicator function that takes the value of 1 if the condition in parentheses holds and 0 otherwise. The first line of Eq. (2) corresponds to firms above the cutoff, and the second line to firms below the cutoff. h is the bandwidth. We use one common bandwidth that optimizes the mean squared error (MSE) for the RD treatment effect estimator following Calonico et al. (2014, 2018).

$K(.)$ is the kernel function. We use a triangular kernel function, $K(u) = (1 - |u|)1(|u| \leq 1)$, because when used in conjunction with the chosen bandwidth, it leads to a point estimator with optimal properties. This kernel function assigns zero weight to all observations with values outside the bandwidth and positive and linear weights to all observations within the bandwidth.

Z is a vector of predetermined covariates before the intervention occurs. For this, we include firm age, a dummy variable that takes the value of 1 if a firm is located in Lima, which is the capital city of Peru, and a dummy variable that takes the value of 1 if the firm belongs to the manufacturing industry.

5. Data

We use *Encuesta Económica Anual* (EEA) (INEI, 2021a) data, which is a yearly enterprise survey in Peru. It is a sample of firms registered in the Peruvian tax authority directory. The sampling frame comprises firms with net annual sales above 150 UIT. The sample is probabilistic, stratified, single-stage and independent in each of the 25 regions of Peru. The firms were selected by systematic sampling with a random starting point. EEA was designed to collect financial and economic data to construct the national account system and, in particular, contained detailed information on Peruvian formal firms' income statements and balance sheets. We have yearly data from 2015 to 2018. Each year has around ten thousand observations, of which we select six thousand that are close to the cutoff using the Calonico et al. (2014) bandwidth selection procedure.

The total annual firm revenue is the running variable. It consists of the sum of sales, interest income, and other income at the end of the year. We use this definition because Peruvian tax authorities use it to identify firms that have to submit the TP report according to the tax reform. According to the reform, firms that had controlled transactions above 400 UIT during the year were subject to the policy. However, we do not observe the flow of controlled transactions during the year, but only the stock at the end of the year in the balance sheet. To overcome this issue, we first study three populations of firms: 1) firms with a positive value in their controlled transactions, 2) all firms regardless of the value of their controlled transactions, and 3) firms with more than 400 UIT in their controlled transactions.

The main outcome variable is tax payment divided by payroll, which is a standard variable to identify the magnitude of profit shifting according to the literature (Bustos et al., 2019, 2022).[4] Tax payments (or income tax) are taxes paid for firms that generate business income. Payroll is the total expenditure on workers, including wages and social security. Some firms set up a company in a country but with almost zero or zero workers. We avoid this as these are usually firms that do not have any economic activity (Carrillo et al., 2022). For this reason, we scaled our tax payment variable by payroll. As mentioned in the context section, we also analyze the spending on consultancy services to evaluate a potential channel that could explain tax avoidance practices. For this, we include the value of the consultancy and advisory services divided by payroll. These outcomes are reported in the income statement.

We present descriptive statistics of the main variables by relevant populations. Table 2 shows the average values for all firms, affected firms and those that are not affected. Affected firms are those with annual revenue higher than 2300 UIT. Panels (A), (B) and (C) show the three ways of approximating the treated firms by controlled transactions. Almost all variables are higher for affected firms.

Panel (A) shows several key results. The revenues of firms are naturally higher because of the definition of the treatment variable. Overall, revenues were about S/ 98 million on average for firms with positive values in their controlled transactions. Moreover, on average, tax payments were around S/ 2.2 million. Furthermore, firms affected by the policy and firms that are not affected pay, on average, 2% of their revenue in taxes. However, affected firms have a higher tax payments/payroll (0.2) than not affected ones (0.08). In addition, the expenditure on consultancy services is higher for affected firms compared to those that are not affected. Treated firms are older and more likely to be in the manufacturing sector.

Panels (B) and (C) show other definitions of the treatment group. Firms, regardless of CT, are 9885, and the proportion of affected firms is around 55%, while the proportion of affected firms among those with controlled transactions greater than 0 or 400 UIT is significantly higher (more than 80%). This is because some small firms did not report the controlled transaction variable. Therefore, when restricting the sample according to the controlled transaction variable, bigger firms are more likely to surpass the threshold of controlled transactions than smaller firms. As bigger firms are affected, the proportion is higher for this group. Another critical difference is that firms regardless of CT and firms with CT greater than 400 UIT are less likely to be in the manufacturing sector if they received the intervention.

6. Results

6.1. Effects on tax outcomes

We first perform a graphical analysis through RD plots. Fig. 3 shows data-driven RD plots following Calonico et al., (2015) procedure. For this, we draw the average of each outcome using binned means over total revenue near the cutoff. RD plots have evenly spaced bins that mimic the underlying variability of the data. The horizontal axis is in millions of Peruvian Soles centered at zero, which equals the cutoff point (2300 UIT). We first restrict the sample within bandwidth and estimate a linear fit below and above the cutoff point. Panels (A), (B) and (C) show three ways to identify treated firms according to the controlled transactions. In all cases, there is a clear discontinuity around the cutoff. In particular, firms just above the threshold have higher income tax/payroll than firms just below the cutoff, suggesting some evidence in favor of the tax reform beforehand.

Table 3 shows the RD coefficients on tax payments/payroll by different levels of the optimal bandwidth. Column (1) shows half the bandwidth, column (2) shows 0.75 of the bandwidth, column (3) shows exactly the bandwidth, column (4) shows 1.25 of the bandwidth, and (5) shows 1.5 of the bandwidth. In addition, we show RD coefficients for the three populations of firms that are likely affected by the policy in panels (A), (B) and (C). The tax reform increased tax payments/payroll by 0.330, which is statistically significant at the 5% level in firms with positive controlled transactions. In 2016, the average value of the tax revenue/payroll was

[4] We also investigate other tax revenue measures such as the probability of paying taxes and the effective tax rate at the firm level.

Table 2
Descriptive statistics.

Variables	All firms		Firms affected		Firms not affected	
(A) Firms with CT >0 UIT						
Total revenue	97.9	(5.3)	112.1	(6.1)	5	(0.1)
Controlled transactions (CT)	48.6	(2.9)	55.6	(3.4)	2.9	(0.4)
Income tax	2.2	(0.2)	2.5	(0.2)	0.1	(0.0)
Payroll	11.2	(0.4)	12.7	(0.4)	1.2	(0.1)
Consultancy services	2.1	(0.2)	2.4	(0.3)	0.2	(0.0)
Firm age	19.3	(0.2)	19.9	(0.2)	14.9	(0.5)
Firms in the capital city, %	74.4	(0.6)	75.9	(0.6)	64.5	(1.7)
Manufacturing firms, %	17.7	(0.5)	18.4	(0.5)	12.8	(1.2)
Observations	6008		5214		794	
(B) Firms regardless of CT						
Total revenue	60.4	(3.3)	111.3	(6.0)	2.5	(0.0)
Controlled transactions (CT)	47.5	(2.9)	55.4	(3.4)	2.5	(0.4)
Income tax	1.3	(0.1)	2.5	(0.2)	0.1	(0.0)
Payroll	7	(0.2)	12.6	(0.4)	0.5	(0.0)
Consultancy services	1.3	(0.1)	2.4	(0.3)	0.1	(0.0)
Firm age	16.7	(0.1)	19.9	(0.2)	13	(0.2)
Firms in the capital city, %	70.1	(0.5)	75.8	(0.6)	63.7	(0.7)
Manufacturing firms, %	21.1	(0.4)	18.3	(0.5)	24.3	(0.6)
Observations	9885		5260		4625	
(C) Firms with CT >400 UIT						
Total revenue	114.1	(6.3)	121	(6.7)	7.1	(0.1)
Controlled transactions (CT)	57.4	(3.5)	60.7	(3.7)	6.7	(1.0)
Income tax	2.6	(0.2)	2.7	(0.2)	0.2	(0.1)
Payroll	13	(0.5)	13.7	(0.5)	1.8	(0.1)
Consultancy services	2.5	(0.3)	2.6	(0.3)	0.4	(0.0)
Firm age	19.9	(0.2)	20.2	(0.2)	15.7	(0.7)
Firms in the capital city, %	77.5	(0.6)	77.5	(0.6)	77.7	(2.4)
Manufacturing firms, %	19.9	(0.6)	19.5	(0.6)	25.9	(2.5)
Observations	5075		4766		309	

Notes: Amounts in million Nuevos Soles (S/) except in firm age and those with %. Controlled transactions (CT) refer to the absolute value of related-party payable plus the absolute value of related-party receivable at the end of 2017. UIT are tax units with the following equivalence in 2017: 1 UIT = S/ 4050. Panels (A), (B) and (C) show different ways to identify treated firms. Firms affected are those with total revenue greater than 2300 UIT. Standard errors are in parentheses.
Source: INEI (2021a).

0.819. As the effect of the tax reform is 0.33, the impact for an average firm close to the cutoff would be an increase of 40% relative to their initial tax payment level.

Narrower or wider bandwidths result in similar point estimation. However, half the bandwidth, or 0.75 of the bandwidth, is not always statistically significant. The effects are similar in magnitude and statistically significant when performing the robust RD coefficient that corrects for misspecification errors. As our results within the three populations using the optimal bandwidth are similar, our preferred specification is firms with positive controlled transactions. Therefore, we only analyze this population in the following parts.

The effect of the policy could be biased by the magnitude of the payroll. Therefore, we also evaluate the effect of the tax reform on alternative tax revenue measures. First, we include as a dependent variable the probability of paying any positive amount of taxes. Around 80% of the firms in the sample paid taxes in 2017. We also include the effective tax rate as an outcome variable. Its average value in 2017 was 2%. Lastly, we evaluated the effect on tax payments scaled by assets. For all three cases, we do not find compelling evidence that there was an effect of the policy on those variables (see Table 9 in the Appendix). Payroll changes do not drive our results, as we also estimated this scenario and found no significant effects (see Table 10 in the Appendix).

We conduct robustness checks to validate the RD estimation in the Appendix. First, we perform a placebo test in which we change the cutoff at different places within the assignment variable. Consistent with the RDD assumption, we don't find any significant effect. Then, we conduct a balance test. We found that the firms are balanced across sectors, age, geographic location, and foreign ownership status.

Fig. 4 shows the local average treatment effects by year from 2015 to 2018. The horizontal axis is the analyzed year, and each point estimation is the RD coefficient using Eq. (1). The effect is nearly zero in 2015 and positive, but with the confidence interval overlapping the value of zero (horizontal red line) in 2016, which suggests that it is not statistically significant. In 2017, the coefficient was higher. However, in 2018, the coefficient was clearly negative.

Table 4 conducts an RD estimation by year. We confirm there was no tax payments/payroll effect in the years before the tax reform took place. This is consistent with the fact that firms did not anticipate the intervention and that the behavior of firms only started to change in 2017.

The average treatment effect is negative and statistically significant at the 10% level in 2018. We performed regression analysis using wider and narrower bandwidths and other population definitions (firms with CT of more than 400 UIT and firms regardless of

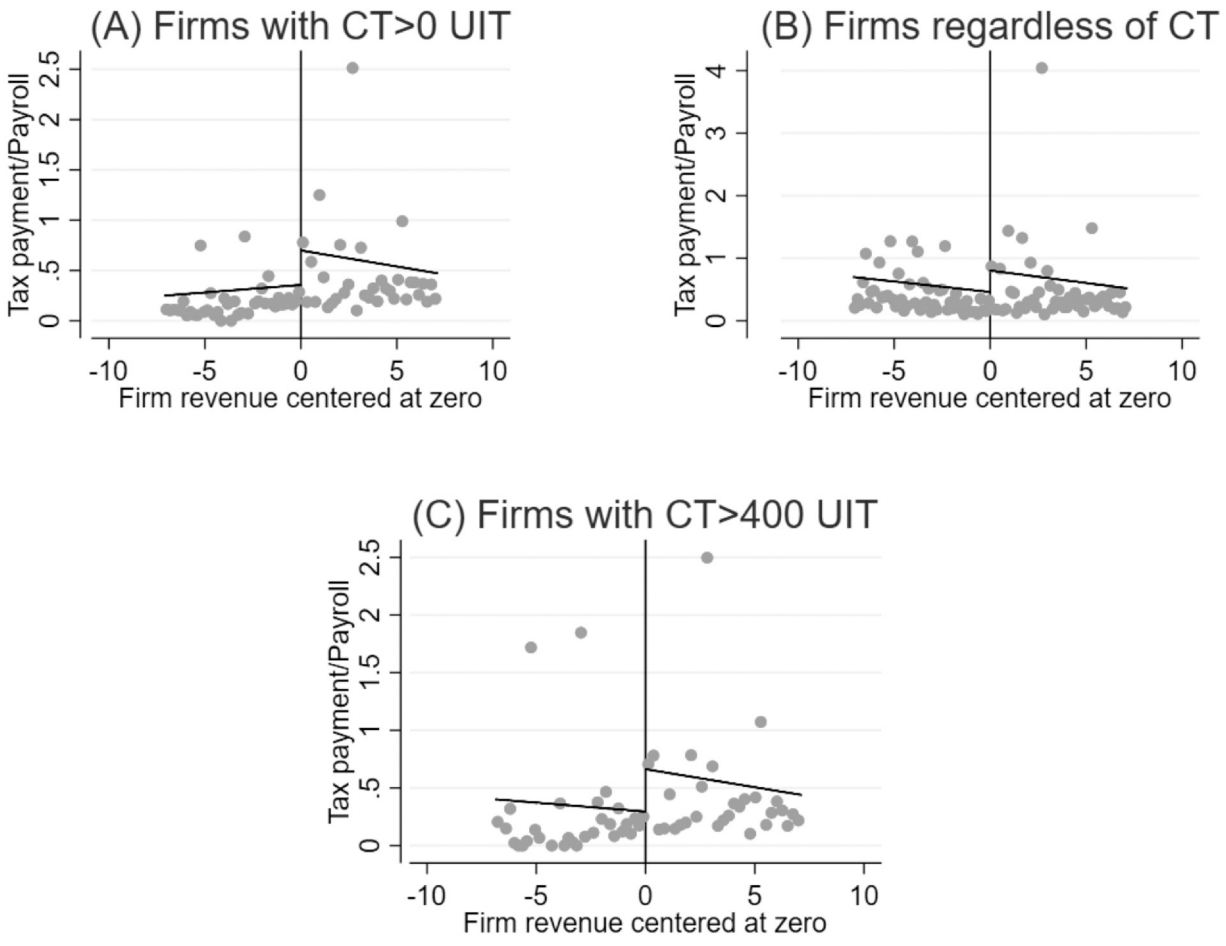

Fig. 3. Regression discontinuity plots on tax payments/payroll, 2017. *Notes:* Data-driven regression discontinuity plots following Calonico et al., (2015) methodology. The solid lines are linear fits. Firm's revenue is measured in millions of Peruvian Soles. It was subtracted from the cutoff point value centered at zero by which firms are subject to the tax reform. The cutoff is S/ 9.315 million (around US$ 2.86 million). Only firms within the optimal bandwidth are shown. Controlled transactions (CT) refer to the absolute value of related party payable plus the absolute value of related party receivable at the end of 2017. Panels (A), (B) and (C) show three ways to identify treated firms according to controlled transactions.

the amount of CT). We found that the effect is no longer significant, even at 10%. Hence, the tax reform lost effectiveness one year later.[5] This is consistent with the fact that the business tax revenue to GDP ratio was constant during the period 2016–2019 (see Fig. 7 in the Appendix). Moreover, tax advisory firm leaders also pointed out that the tax reform was ineffective (Aragón and Hinostroza, 2019). Similar results are shown in Wier (2020) in the context of a similar tax reform in South Africa. The author found that a transfer price reform that increases documentation requirements was ineffective three years after the beginning of the policy.

The fact that the reform is not working after one year might indicate that firms realize that the enforcement capacity of the tax authority did not increase. Hence, they returned to undertaking elusive practices. According to Pomeranz (2015) and Casey and Castro (2015), actions taken by tax authorities are key in the firm perceptions of enforcement. Peru's resources invested in monitoring BEPS decreased slightly from 2016 to 2017. Fig. 8 in the Appendix shows that, in 2017, there were 350,826 inspection and warning actions, which was 7.6% fewer actions than in 2016. Therefore, firms could have realized that tax authorities only applied a modification in law (*de jure*) but not in practice (*de facto*). Even if tax authorities have more information and power to force firms to pay their taxes, policies will not work if they do not have more resources to increase enforcement actions effectively.

Moreover, firms might have learnt more about the tax reform and how to operate within this framework with a low tax payment. Firms might be behaving differently in 2018 as a result of the reform that took place in 2017. We hypothesize that firms invest more in consultancy to adapt their tax reporting practices to the new law. As a result of this investment, firms could have paid fewer taxes, suggesting that they found ways to increase tax avoidance.

[5] Table 10 in the Appendix shows that our results in 2018 are also not driven by payroll.

Table 3
Local average treatment effects on tax payments/payroll by bandwidth.

	(1)	(2)	(3)	(4)	(5)
	x0.5	x0.75	x1	x1.25	x1.5
(A) Firms with CT >0 UIT					
β_{RD}	0.266	0.320 **	0.330 **	0.324 **	0.319 **
	(0.231)	(0.162)	(0.153)	(0.156)	(0.154)
Observations	970	1323	1694	2196	2441
Bandwidth	3.562	5.343	7.124	8.905	10.686
(B) Firms regardless of CT					
β_{RD}	0.394 *	0.352	0.342 *	0.396 **	0.404 ***
	(0.224)	(0.216)	(0.197)	(0.163)	(0.156)
Observations	699	1045	1406	1829	2318
Bandwidth	2.047	3.071	4.095	5.119	6.142
(C) Firms with CT >400 UIT					
β_{RD}	0.235	0.315 *	0.347 *	0.358 *	0.350 **
	(0.181)	(0.162)	(0.180)	(0.183)	(0.177)
Observations	853	1183	1497	1744	1981
Bandwidth	4.759	7.138	9.517	11.896	14.276

Notes: Coefficients from RD estimation using Eq. (1) are shown. The outcome variable is tax payments/payroll in 2017. The running variable is the firm's revenue (in millions of Peruvian Soles) in 2017 with the cutoff given by the tax reform centered at zero. The optimal bandwidth is determined according to Calonico et al. (2014). The columns show the results according to different bandwidths: column (1) shows half the bandwidth, column (2) 0.75 of the bandwidth, and so on. Panels (A), (B) and (C) show three ways to identify treated firms according to controlled transactions. Standard errors are in parentheses. *Significant at 10%, **significant at 5%, ***significant at 1%.

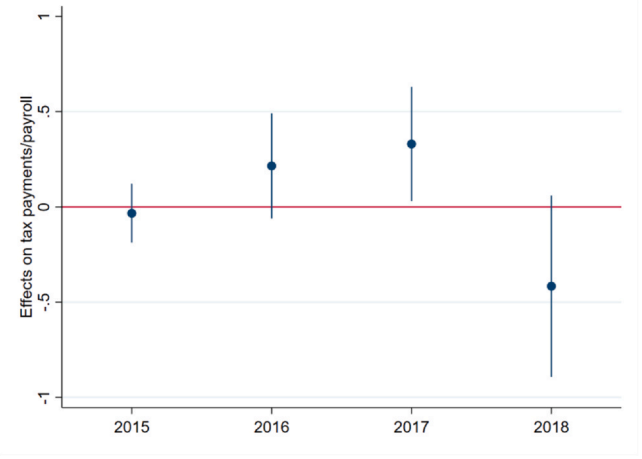

Fig. 4. Local average treatment effect on tax payments/payroll by year. *Notes:* Regression discontinuity coefficient on tax payments/payroll using the firms' revenue as the running variable and the cutoff given by the tax reform are shown for each year. Vertical lines are confidence intervals at 95%.

Table 4
Local average treatment effects on tax payments/payroll by year.

	(1)	(2)	(3)	(4)
	2015	2016	2017	2018
β_{RD}	-0.033	0.215	0.330 **	-0.417 **
	(0.079)	(0.141)	(0.153)	(0.243)
Observations	1755	1317	1694	729
Bandwidth	6.348	4.329	7.124	3.591

Notes: Coefficients from an RD estimation using Eq. (1) are shown. The outcome variable is tax payments/payroll. The running variable is the firm's revenue (in millions of Peruvian Soles) with the cutoff given by the tax reform centered at zero. The optimal bandwidth is determined according to Calonico et al. (2014). The columns show the results for different years. Standard errors are in parentheses. *Significant at 10%, **significant at 5%, ***significant at 1%.

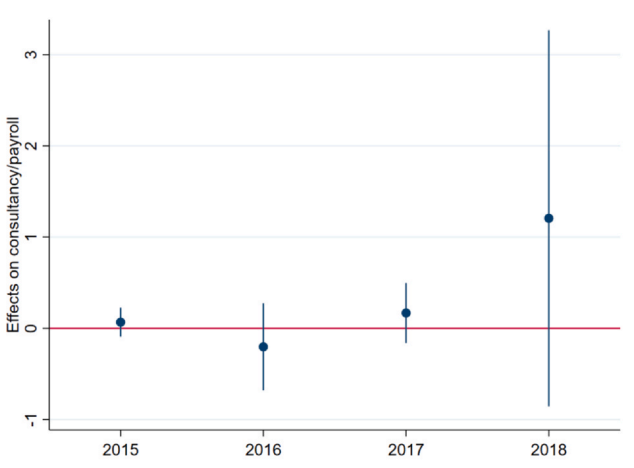

Fig. 5. Local average treatment effect on consultancy/payroll by year. *Notes:* Regression discontinuity coefficient on consultancy expenditure/payroll using the firms' revenue as the running variable and the cutoff given by the tax reform are shown for each year. Vertical lines are confidence intervals at 95%.

Table 5
Local average treatment effects on consultancy/payroll by year.

	(1) 2015	(2) 2016	(3) 2017	(4) 2018
β_{RD}	0.067	-0.203	0.168	1.206
	(0.081)	(0.243)	(0.168)	(1.052)
Observations	847	1283	1272	797
Bandwidth	2.328	4.183	5.134	3.842

Notes: Coefficients from RD estimation using Eq. (1) are shown. The outcome variable is tax payments/payroll. The running variable is the firms' revenue (in millions of Peruvian Soles) with the cutoff given by the tax reform centered at zero. The optimal bandwidth is determined according to Calonico et al. (2014). The columns show the results for different years. Standard errors are in parentheses. *Significant at 10%, **significant at 5%, ***significant at 1%.

6.2. Role of tax consultancy services

We conduct a graphical analysis of the tax reform's effect on the ratio of consultancy services/payroll by year in Fig. 5. We confirm that the local average treatment effect is nearly zero from 2015 to 2017. Nevertheless, in 2018, the point estimation is positive but noisier. We further analyze these results using RD estimation.

Table 5 shows the effect of the intervention on consultancy/payroll. It is shown that, in all cases, the effect on all firms is not statistically significant. We confirm that the coefficient is positive but with a higher standard error in 2018.

We investigate if there are heterogeneous effects depending on the intensity of consultancy expenditure relative to the payroll. For this, we restrict the sample to four quartiles of the distribution of consultancy expenditure in 2018. We focus on 2018 because we want to explain why we have negative effects in 2018 if we had positive effects in 2017. We found little evidence that firms with greater consultancy expenditures reduced their tax payment. Table 11 in the Appendix shows that firms above the 4th quartile of the distribution of consultancy expenditures reduced their tax payment by -0.974, significant at the 10% level.

Another reason why we did not find effects in 2018 is that firms might have increased their expenses. The evidence suggests that some policies that increase tax enforcement or reporting did not increase tax revenue significantly because firms responded to the policy by increasing their expenses, so additional net tax collection is minimal (Carrillo et al., 2017; Adhikari et al., 2022). For this, we evaluate the effect on different types of expenses. First, intermediate consumption includes the expenses for inputs used in the process of production. Second, impairment assets are losses in the value of assets within the current period, such as depreciation. Third, interest expenses are incurred by the firm for borrowed funds. We did not find any effect on either of these outcomes in 2018 (see Table 12 in the Appendix).

Finally, we analyzed heterogeneous effects across the distribution of each type of expense. We found that firms above the 4th quartile dropped their effective tax rate by -0.023, which is significant at the 5% level (see Table 13 in the Appendix). The effect of the tax reform was also negative for firms in the 1st quartile (see Table 14 in the Appendix), but this was only statistically significant at the 10% level. Finally, we found consistent results for firms that spent less on interest expenses. The probability of paying taxes and the effective tax rate were 0.646 and 0.027, respectively, and statistically significant at 1% (see Table 15 in the Appendix). We interpret this result as follows. Firms that are low spenders in interest expenses increased their tax payments due to the tax reform. We could not find evidence that the tax reform worked for high spenders in interest expenses, which is consistent with the fact that firms could increase these expenses to reduce their tax base and pay less taxes.

7. Conclusion

In this paper, we estimated the causal effect of a tax reform that increased the cost of avoiding taxes on Peruvian firms' tax payments. Given the vital role accounting firms play in tax avoidance schemes, we included the effect of expenditure on tax advisory. We considered the possibility that Peruvian firms could invest in tax planning services to continue avoiding taxes. This approach allowed us to investigate the channels used by multinationals to lower their tax payments. We found that firms (with annual revenues near 2300 UIT) affected by the intervention paid more taxes on average in 2017. Therefore, we confirmed that the tax reform affected tax payments positively and might contribute to reducing tax avoidance. In this regard, policies that boost tax transparency and strengthen the institutional capacities of the tax authorities are essential.

We also analyzed a potential channel of tax avoidance for tax consultancy services. Firms might want to invest in tax planning and advisory services to continue avoiding tax payments. We found that the tax reform did not significantly affect tax payments one year after it came into force, that is, in 2018. However, when analyzing the tax reform's effect on consultancy services in 2018, we found tentative but insignificant effects. More research should be done to disentangle the role of tax advisory firms in tax avoidance.

The OECD/G20 Inclusive Framework on Base Erosion and Profit Shifting is advancing proposals that may transform international taxation with potentially large implications for developing countries. Those effects have to be analyzed. Clearly, with an extensive list of Sustainable Development Goals to finance, it is important for governments in developing countries to ensure that their current and future tax regimes and tax reforms contribute fully to government budgets.

Appendix A. Robustness check

The first robustness check is changing the cutoff to the left and right of the assignment variable. The cutoff point should generate an exogenous shift attributable to the policy. If other cutoff points generate an unexplained effect on our outcome, then our RDD might not be valid. Table 6 performs an RD estimation using cutoff points different from the one of the tax reform. Column (1) shows the result from a cutoff equal to the median of the values to the left of the benchmark cutoff. Column (2) shows the result from the benchmark cutoff, that is, the one specified by the reform. Column (3) shows the result from a cutoff equal to the median of the values to the right of the benchmark cutoff. As can be seen, no effects were found by cutoffs different from the ones mandated by the tax reform.

Another robustness check to validate our identification strategy is the balance test (Lee and Lemieux, 2010). We conduct a graphical and regression analysis using outcomes different from tax payments/payroll. If firms just above and below the cutoff point are balanced in their covariates, then the policy is locally randomized around the threshold. Fig. 6 shows RD plots using as outcome variables: a dummy if the firm is in the primary sector, manufacturing sector, capital city, a domestic private firm, and years of operations. In almost all cases, the linear fit seems continuous around the cutoff.

Table 7 shows the regression discontinuity coefficients on previous outcomes. Tax payments/payroll is the only outcome with a statistically significant coefficient. The probability of being a manufacturing firm in the capital city, or firm age, has a coefficient of zero because we are already controlling for those variables in the main regression. For this reason, we confirm that firms are balanced around the cutoff.

We also include a sensitivity analysis using kernel distributions other than triangular. Using the standard linear function (uniform), we found significant effects when narrowing the bandwidth 0.75 times the optimal bandwidth. As this kernel function treats firms far from the cutoff equally to those that are very close, we are likely introducing biases. Then, using the Epanechnikov function, we again found significant positive effects. This is a consistent result as this function (similar to triangular) also prioritizes firms that are close to the bandwidth.

Table 6
Local average treatment effects on tax payments/payroll by other cutoffs.

	(1) At left median	(2) At zero	(3) At right median
β_{RD}	-0.084	0.330 **	-0.353
	(0.095)	(0.153)	(0.325)
Observations	1657	1694	2044
Bandwidth	7.124	7.124	7.124

Notes: Coefficients from RD estimation using Eq. (1) are shown. The outcome variable is tax payments/payroll in 2017. The running variable is the firm's revenue (in millions of Peruvian Soles) in 2017 with the cutoff given by the tax reform centered at zero. The optimal bandwidth is determined according to Calonico et al. (2014). The columns show the results according to different cutoffs: column (1) shows a cutoff to the left, column (2) shows the benchmark cutoff, and column (3) shows a cutoff to the right. Standard errors are in parentheses. *Significant at 10%, **significant at 5%, ***significant at 1%.

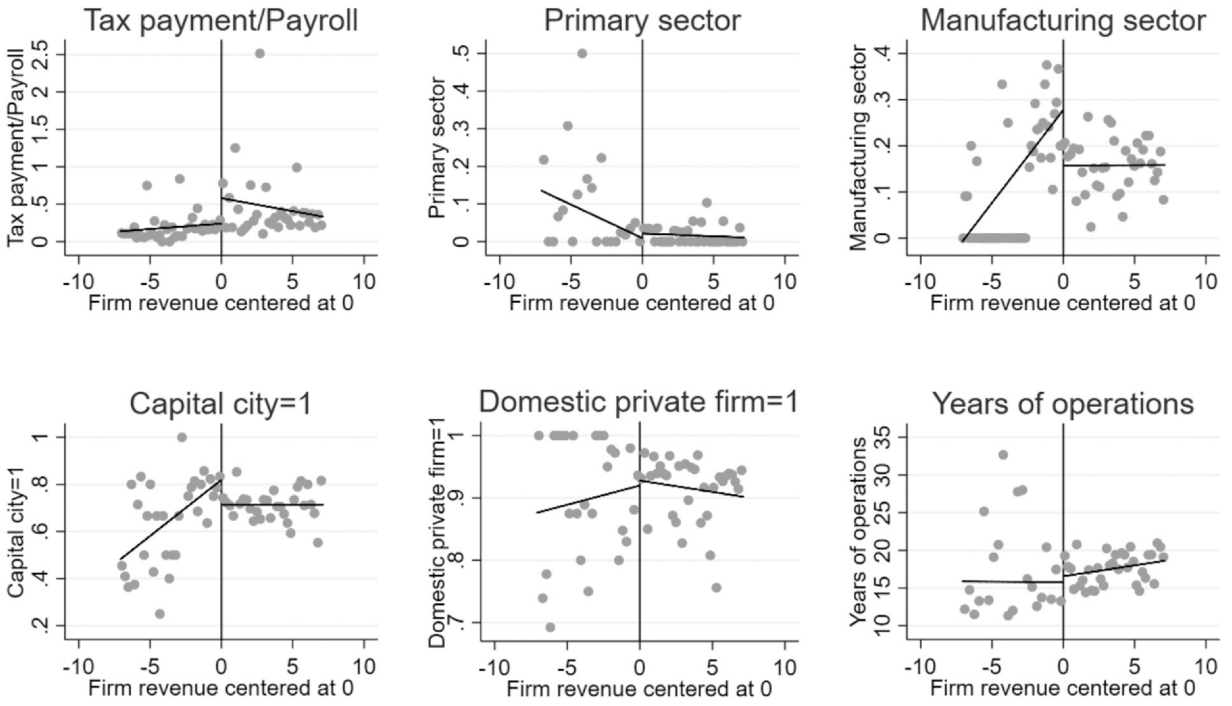

Fig. 6. Regression discontinuity plot on different outcomes. *Notes:* Data-driven regression discontinuity plots following Calonico et al., (2015) methodology. The solid lines are linear fits. Firm's revenue is measured in millions of Peruvian Soles. It was subtracted from the cutoff point value centered at zero by which firms are subject to the tax reform. The cutoff is S/ 9.315 million (around US$ 2.86 million). Only firms within the optimal bandwidth are shown.

Table 7
Local average treatment effects on other outcomes (Balance test).

	(1) Tax/w	(2) Extraction	(3) Manufacturing	(4) Capital city	(5) Domestic private	(6) Firm age
β_{RD}	0.330 **	0.009	-0.000	-0.000	0.033	-0.000
	(0.153)	(0.015)	(0.000)	(0.000)	(0.026)	(0.000)
Observations	1694	1712	1712	1712	1712	1712
Bandwidth	7.124	7.124	7.124	7.124	7.124	7.124

Notes: Coefficients from RD estimation using Eq. (1) are shown. The columns show different outcome variables of firms in 2017. The running variable is the firm's revenue (in millions of Peruvian Soles) in 2017 with the cutoff given by the tax reform centered at zero. The optimal bandwidth is determined according to Calonico et al. (2014). Standard errors are in parentheses. *Significant at 10%, **significant at 5%, ***significant at 1%.

Appendix B. Additional tables and figures

See Appendix B Figs. 7 and 8, Tables 8–15 here.

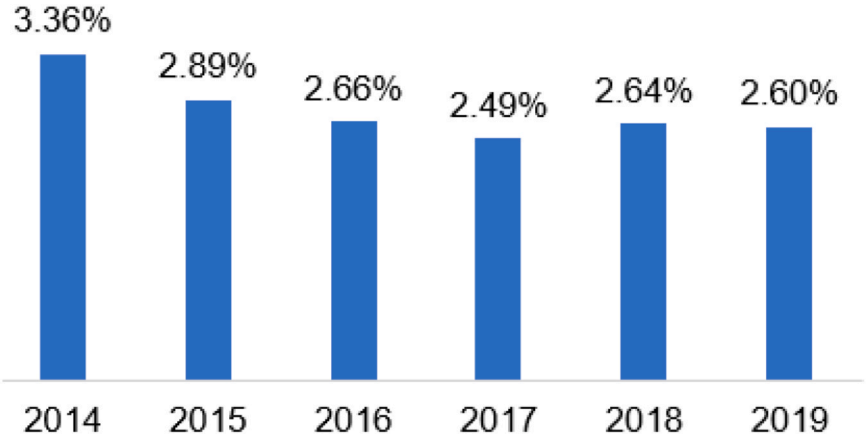

Fig. 7. Business tax revenue to GDP ratio by year. *Note:* Only business income from the private sector is included.
Source: INEI (2021b).

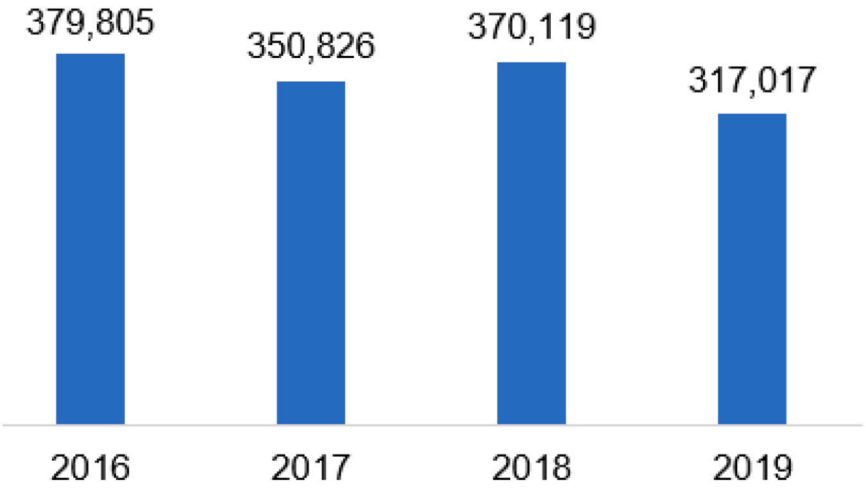

Fig. 8. Audit actions by year. *Note:* Audit actions refer to actions that seek to promote tax compliance through inspections and warnings.
Source: SUNAT (2020).

Table 8
Local average treatment effects on tax payments by kernel distributions.

	(1)	(2)	(3)	(4)	(5)
	x0.5	x0.75	x1	x1.25	x1.5
Triangular	0.266	0.320 **	0.330 **	0.324 **	0.319 **
	(0.231)	(0.162)	(0.153)	(0.156)	(0.154)
Bandwidth	3.562	5.343	7.124	8.905	10.686
Observations	970	1323	1694	2196	2441
Uniform	0.403	0.474 **	–0.006	0.208	0.331 **
	(0.253)	(0.226)	(0.445)	(0.222)	(0.160)
Bandwidth	1.383	2.075	2.767	3.459	4.150
Observations	449	667	819	949	1084
Epanechnikov	0.442 **	0.224	0.292 *	0.331 **	0.334 **
	(0.223)	(0.247)	(0.172)	(0.155)	(0.160)
Bandwidth	2.335	3.502	4.669	5.837	7.004
Observations	731	957	1187	1440	1665

Notes: Coefficients from an RD estimation using Eq. (1) are shown. The outcome variable is tax payments. The running variable is the firm's revenue (in millions of Peruvian Soles) with the cutoff given by the tax reform centered at zero. The optimal bandwidth is determined according to Calonico et al. (2014). Consultancy exp. above p50 are firms with consultancy expenditures above the median, and below p50 are those below the median. Standard errors are in parentheses. *Significant at 10%, **significant at 5%, ***significant at 1%

Table 9
Local average treatment effects on other tax revenue measures.

	(1)	(2)	(3)	(4)	(5)
	x0.5	x0.75	x1	x1.25	x1.5
Tax payment/Payroll	0.266	0.320 **	0.330 **	0.324 **	0.319 **
	(0.231)	(0.162)	(0.153)	(0.156)	(0.154)
Bandwidth	3.562	5.343	7.124	8.905	10.686
Observations	970	1323	1694	2196	2441
Probability of paying taxes	0.023	0.026	–0.002	–0.015	–0.019
	(0.063)	(0.054)	(0.047)	(0.041)	(0.037)
Bandwidth	2.760	4.139	5.519	6.899	8.279
Observations	824	1089	1371	1669	2002
Effective tax rate	0.003	0.002	–0.003	–0.007 **	–0.009 *
	(0.007)	(0.006)	(0.003)	(0.004)	(0.005)
Bandwidth	3.248	4.872	6.497	8.121	9.745
Observations	923	1244	1576	1957	2347
Tax payment/Assets	–0.001	–0.002	–0.002	–0.002	–0.003
	(0.006)	(0.005)	(0.004)	(0.004)	(0.004)
Bandwidth	3.467	5.200	6.934	8.667	10.401
Observations	961	1306	1671	2133	2430

Notes: Coefficients from an RD estimation using equation 3 are shown. The running variable is the firm's revenue (in millions of Peruvian Soles) with the cutoff given by the tax reform centered at zero. The optimal bandwidth is determined according to Calonico et al. (2014). Standard errors are in parentheses. *Significant at 10%, **significant at 5%, ***significant at 1%

Table 10
Local average treatment effects on payroll by bandwidth.

	(1)	(2)	(3)	(4)	(5)
	x0.5	x0.75	x1	x1.25	x1.5
Payroll in 2017	-0.249	-0.036	0.101	0.158	0.176
	(0.267)	(0.233)	(0.206)	(0.185)	(0.171)
Observations	788	1029	1285	1557	1848
Bandwidth	2.553	3.829	5.105	6.381	7.658
Payroll in 2018	0.090	0.267	0.195	0.193	0.203
	(0.497)	(0.393)	(0.336)	(0.299)	(0.268)
Observations	325	517	735	974	1275
Bandwidth	1.760	2.639	3.519	4.399	5.279

Notes: Coefficients from RD estimation using Eq. (1) are shown. The outcome variable is payroll and the running variable is the firm's revenue, both in millions of Peruvian Soles, with the cutoff given by the tax reform centered at zero. The optimal bandwidth is determined according to Calonico et al. (2014). The columns show the results according to different bandwidths: column (1) shows half the bandwidth, column (2) shows 0.75 of the bandwidth, and so on. Panels (A), (B) and (C) show three ways to identify treated firms according to controlled transactions. Standard errors are in parentheses. *Significant at 10%, **significant at 5%, ***significant at 1%.

Table 11
Local average treatment effects across the intensity of consultancy expenses on tax revenue, 2018.

	(1)	(2)	(3)	(4)
	p25 or less	p25-p50	p50-p75	p75 or more
Tax payment/Payroll	–1.840	0.025	–0.055	–0.974 *
	(1.875)	(0.088)	(0.081)	(0.573)
Bandwidth	2.286	8.338	8.332	3.618
Observations	85	777	880	198
Probability of paying taxes	0.239	0.041	–0.040	–0.039
	(0.235)	(0.117)	(0.105)	(0.149)
Bandwidth	2.582	9.169	7.038	4.530
Observations	95	1128	559	275
Effective tax rate	–0.012	0.008	0.004	–0.012
	(0.013)	(0.006)	(0.007)	(0.011)
Bandwidth	3.492	7.418	8.792	3.719
Observations	148	557	1076	210

Notes: Coefficients from RD estimation using Eq. (1) are shown. Each column shows the policy effect across quartiles of consultancy expenses. The running variable is the firm's revenue (in millions of Peruvian Soles) with the cutoff given by the tax reform centered at zero. The optimal bandwidth is determined according to Calonico et al. (2014). Standard errors are in parentheses. *Significant at 10%, **significant at 5%, ***significant at 1%

Table 12
Local average treatment effects on other firm expenses, 2018.

	(1)	(2)	(3)	(4)	(5)
	x0.5	x0.75	x1	x1.25	x1.5
Intermediate consumption	–4.041	–4.186	0.275	1.836	1.937
	(5.332)	(4.448)	(3.651)	(3.277)	(3.190)
Bandwidth	1.901	2.852	3.802	4.753	5.703
Observations	343	559	788	1072	1419
Impairment assets	–0.677	–0.607	–0.443	–0.374	–0.465
	(1.137)	(0.875)	(0.700)	(0.573)	(0.508)
Bandwidth	1.894	2.841	3.788	4.735	5.682
Observations	343	555	783	1070	1410
Interest expense	–1.360 *	–0.656	–0.520	–0.583	–0.350
	(0.806)	(0.512)	(0.621)	(0.794)	(0.701)
Bandwidth	2.774	4.161	5.548	6.935	8.322
Observations	543	876	1359	2035	3463

Notes: Coefficients from RD estimation using Eq. (1) are shown. The running variable is the firm's revenue (in millions of Peruvian Soles) with the cutoff given by the tax reform centered at zero. The optimal bandwidth is determined according to Calonico et al. (2014). Standard errors are in parentheses. *Significant at 10%, **significant at 5%, ***significant at 1%

Table 13
Local average treatment effects across the intensity of intermediate consumption on tax revenue, 2018.

	(1)	(2)	(3)	(4)
	p25 or less	p25-p50	p50-p75	p75 or more
Tax payment/Payroll	0.078	–0.121	0.060	–2.263
	(0.071)	(0.075)	(0.071)	(1.422)
Bandwidth	5.710	4.998	5.671	5.050
Observations	335	280	368	301
Probability of paying taxes	0.100	0.064	0.023	–0.097
	(0.146)	(0.135)	(0.183)	(0.133)
Bandwidth	5.275	6.959	4.229	6.709
Observations	292	476	241	501
Effective tax rate	0.010	–0.008	0.003	–0.023 **
	(0.010)	(0.009)	(0.008)	(0.011)
Bandwidth	5.360	4.748	5.000	5.300
Observations	300	263	302	331

Notes: Coefficients from RD estimation using Eq. (1) are shown. Each column shows the policy effect across quartiles of intermediate consumption. The running variable is the firm's revenue (in millions of Peruvian Soles) with the cutoff given by the tax reform centered at zero. The optimal bandwidth is determined according to Calonico et al. (2014). Standard errors are in parentheses. *Significant at 10%, **significant at 5%, ***significant at 1%

Table 14
Local average treatment effects across the intensity of impairment assets on tax revenue, 2018.

	(1)	(2)	(3)	(4)
	p25 or less	p25-p50	p50-p75	p75 or more
Tax payment/Payroll	–1.270	–0.063	0.134	–1.765
	(0.822)	(0.156)	(0.135)	(1.763)
Bandwidth	4.335	9.069	5.913	9.582
Observations	217	1271	358	1294
Probability of paying taxes	0.235	0.032	0.054	–0.132
	(0.170)	(0.126)	(0.133)	(0.147)
Bandwidth	3.947	5.281	9.124	7.748
Observations	192	352	1164	657
Effective tax rate	–0.019 *	0.011	0.009	–0.007
	(0.011)	(0.008)	(0.008)	(0.010)
Bandwidth	3.330	6.786	7.139	7.356
Observations	154	540	500	575

Notes: Coefficients from RD estimation using Eq. (1) are shown. Each column shows the policy effect across quartiles of impairment asset expenses. The running variable is the firm's revenue (in millions of Peruvian Soles) with the cutoff given by the tax reform centered at zero. The optimal bandwidth is determined according to Calonico et al. (2014). Standard errors are in parentheses. *Significant at 10%, **significant at 5%, ***significant at 1%

Table 15
Local average treatment effects across the intensity of interest expenses on tax revenue, 2018.

	(1)	(2)	(3)	(4)
	p25 or less	p25-p50	p50-p75	p75 or more
Tax payment/Payroll	0.100	–0.000	–0.006	–4.418
	(0.167)	(0.051)	(0.129)	(4.177)
Bandwidth	1.849	3.758	7.705	7.126
Observations	51	236	712	491
Probability of paying taxes	0.646 ***	–0.158	–0.037	0.119
	(0.219)	(0.144)	(0.106)	(0.122)
Bandwidth	2.487	4.873	8.206	10.220
Observations	77	329	844	1004
Effective tax rate	0.027 ***	–0.004	0.011 *	–0.013
	(0.009)	(0.011)	(0.007)	(0.009)
Bandwidth	1.545	3.767	7.798	11.465
Observations	40	237	729	1023

Notes: Coefficients from an RD estimation using Eq. (1) are shown. Each column shows the policy effect across quartiles of interest expenses. The running variable is the firm's revenue (in millions of Peruvian Soles) with the cutoff given by the tax reform centered at zero. The optimal bandwidth is determined according to Calonico et al. (2014). Standard errors are in parentheses. *Significant at 10%, **significant at 5%, ***significant at 1%

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